Mirsky, Allan F. 2001 A

Dr. Allan F. Mirsky Oral History 2001 A

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Allan Mirsky

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This is an interview with Dr. Mirsky, chief of the Clinical and Experimental Neuropsychology Section of the Brain and Cognition Laboratory of the NIMH Intramural Research Program held on October 30, 2001, in Bethesda, Maryland. The principal interviewer is Dr. Ingrid Farreras of the NIH History Office.

Ferreras: So, why don't we start by your introducing yourself by full name and date and place of birth.

Mirsky: My name is Allan Franklin Mirsky, and I was born in Brooklyn, New York, February 2, 1929, on Ground Hog's Day, which has since become a national holiday in my honor. No, not really.

My parents lived in Brooklyn for several years, and then, early in the Depression, moved to Stanford, Connecticut, which was my mother's home, because my father's business had failed. In 1932, we returned to New York City, to the Bronx, and I grew up in the Bronx and lived there from 1932 till 1950.

I was educated in New York City public schools, P.S. 23, P.S. 51, which was junior high school, and then the Bronx High School of Science, and after graduating from the Bronx High School of Science, I went to City College of New York, where I got my bachelor's degree in psychology. Psychology as a career emerged somewhat late in my undergraduate career. I think I started out as an engineer and then shifted into majoring in French, and from there, pre-med, and then biology, and finally psychology. I guess psychology wasn't that clear a career option initially, and it's hard to know what a psychologist did or was in 1947 or '48.

Farreras: Was there anyone in particular who influenced that decision or coursework?

Mirsky: Well, I think it's probably fair to say that once I started to take psychology courses, which I did because I thought they were fun as opposed to some of the other courses, which I knew I had to take in order to get into graduate school or medical school, well, the Psychology Department at CCNY was one of the best in the country at that time. The faculty was outstanding. The chairman of the department, Gartner Murphy, some people regarded as the successor to William James in terms of his depth and breadth. I recall that his lectures, any one of which could have been a *Psych Review* article, they were that good, his lectures were packed – not only with students taking the course, but with, if you had a free hour, you would go to hear Gartner Murphy. He was just that good.

Farreras: Just like today.

Mirsky: Yeah. So Murphy was certainly one of the influences.

But I think that two professors who had the most direct influence on me were Herbert Birch and Joseph Barmack, both professors in the Department of Psychology. Birch was an extremely charismatic character and a very exciting teacher. Barmack was more solid, less flashy. I can recall, for example, when I did decide finally that I was going to go to graduate school in psychology and I applied to a number of places, I received a number of acceptances, including one from Yale University. And when I discussed this with Birch, he said, "Why do you want to go to Yale? That's where Hull is," Clark Hull, who was a learning theorist who represented a kind of view that Birch found unacceptable, shall we say, a kind of mechanistic reflex, Pavlovian type approach to learning, and Birch was more of the gestalt persuasion. Then, when I spoke to Barmack about this, Barmack said, "Of course go to Yale. They have a wonderful research tradition." That was the important thing for him, not the politics of Clark Hull, who really wasn't very active at that time. He was still alive, still at Yale, but his mantle, so to speak, had been taken up by Neil Miller [sp.] and others, John Dollart [sp.] among others.

Farreras: So you obtained your master's and Ph.D. degree at Yale in '52 and '53.

Mirsky: That's right.

Farreras: How did you come about that?

Mirsky: It was not Providence, Rhode Island, by the way. It was New Haven, Connecticut. Yale was in New Haven, Connecticut. I had applied originally... Or, again, the various distinctions in terms of careers in psychology were not terribly clear to me at the time. I think the Boulder model, which Dave Shakow was instrumental in creating, by the way, that Boulder conference was something like 1950.

Farreras: Forty-nine.

Mirsky: Okay, 1949, 1950. And that went a long way towards defining what clinical psychology would be. So I had applied, I guess, to the clinical program and was accepted, and the director of the clinical program, Seymour Sarrasin [sp.], had made his reputation in doing research on mental retardation. He had been the director of psychology at Southberry [sp.] State Training School, and later that was an important connection for me.

Anyway, I remember talking to Seymour and saying that it wasn't clear to me that I wanted to go into clinical psychology as opposed to experimental or physiological, because the stipend, the assistantship I had was working with Haldor Rosvold, and that was doing experimental work rather than clinical work. So Sarrasin's [sp.] advice to me was, "Well, it's easier to get out of clinical than to get into it. If you decide you're going to go experimental at this point and then later change your mind, it's going to be hard to get back into clinical, so why don't you stay where you are," so I did.

Farreras: When you first applied, what made you choose a clinical program, given that it was so amorphous at the time, that clinical education was so amorphous at the time?

Mirsky: That's a good question. I suppose it seemed more diversely relevant to helping people, to working with sick people. God knows there was enough psychopathology in my family, not my immediate family, but I had a grandfather who I used to visit in an institution, a mental hospital, a couple of aunts who were schizophrenic, let's say, and I think that might have been a stimulus for me to become interested.

Farreras: Family.

Mirsky: Yeah. Was there anything that one could do to understand how these people got to be that way, so that in that very amorphous sense, I thought perhaps clinical would be the way to do something with respect to that sort of issue.

Farreras: How did you learn about what schools offered that type of training at the time?

Mirsky: Well, there wasn't very much help from the department. I think, well, if you could call on a professor and he would spend a few minutes with you, you might get some advice. Well, I went to such-and-such university or you might _____. APA I think used to publish yearly – I'm sure they still do – an issue listing graduate programs in psychology.

Farreras: Yeah, they do.

Mirsky: So I applied to a few of them. I applied to Yale, I applied to Indiana, Rochester, Clark, maybe some others, Wisconsin. I applied to Wisconsin, and I applied to Wisconsin I think primarily because Harlow was there and I _____ his work and I said, "Well, that's interesting stuff." Why I chose all the others, I'm not really sure.

Farreras: But you said you obtained an assistantship with Rosvold, who's not in the clinical program. Or was he? How come you didn't get a clinical assistantship? Wasn't the VA funding graduate students at the time to obtain clinical training at Yale and other schools?

Mirsky: I'm not sure whether it was... Yes, it might have been the VA. It might have been NIH, although that might have been too early for NIH to be...

Farreras: Well, USPHS started.

Mirsky: Yeah. There were... Many of the students in the clinical program at Yale were on stipends that were provided by some government agency. It might have been the VA, it might have been the Public Health Service. I did not have one of those. My stipend was through a Veterans Administration grant that was given to the principal investigator. I think that was John Fulton, who was chairman of the Department of Physiology at Yale. And I think Rosvold's salary, although I'm not sure about this, Rosvold's salary might have come in part from that grant. That certainly wouldn't be an unusual arrangement. And Hull was not, as I recall, part of the clinical faculty, but it seemed to me that the walls between programs were more permeable than they are now. I know Hull would lecture from time to time on topics that were of clinical interest, and the research that he did certainly could have been classified as clinical research, studying the effects of prefrontal lesions on cognitive behavior.

Farreras: Is that the work that you were doing with him while you were there, while you were at Yale?

Mirsky: Yes, in the most general sense.

Now, Hull's own research was funded by the Veterans Administration, as was Fulton's. It would be interesting to check with Mort Mishkin because he might have a clearer understanding of exactly what the relationship was between Hull's grant and the larger grant that was given to John Fulton, but, clearly, Yale was the center where there were many studies of the effects of frontal lobe lesions, not just out of the pure scientific interest, but because in the late '40s, there were many hundreds, if not thousands, of veterans returning from World War II who had psychiatric or psychological problems, and the VA was faced with the problem of how to care for them. Back in the '30s, there had been the introduction of the technique of prefrontal lobotomy, which was alleged early on to be a treatment for psychosis, specifically schizophrenia, and, actually, John Fulton had been one of the persons involved in the basic research that gave rise to that. Fulton and Jacobson presented the results at an international meeting of lesions in two chimpanzees, Becky and Lucy, and Becky became more docile following prefrontal lesions and he announced this at a meeting. I think within a year or two, a Portuguese neurosurgeon, Egas Monice [sp.], announced that he had operated 10 schizophrenic patients with the same operation and reported great success. Whether Fulton was shocked or not is not clear, but that launched the lobotomy era, and at the time we had really no treatments, no effective treatments for schizophrenia, and the VA, faced with this massive health problem, was interested in funding research on whether this was an effective method for treatment.

I think a lot of the _____ who were pushed to do psychosurgery research ended when the first successful neuroleptic was introduced. That was chlorpromazine.

Farreras: Mid-'50s, late '50s?

Mirsky: Yeah. I think it was introduced into this country about '55, '56, something like that. And that was something that actually treated the symptoms of schizophrenia.

So, I think the treatments that are offered for schizophrenia are always a reflection of what the related biochemical or biophysical or other research at the time, it's a reflection of what's available. When we were concerned about auto infection as a treatment for schizophrenic, patients were treated by removing the focus of infection, namely, their teeth. So schizophrenics were treated, and are still treated in some parts of this country, by having all their teeth removed. That's the treatment. I'm not sure whether I'd rather lose a part of my frontal lobes or all my _____. I guess I would rather lose my teeth.

So, this was a kind of interesting transitional time, the '50s, with psychosurgery still being practiced and being replaced by pharmacological treatment.

So, anyway, the lobotomy project _____ funded my graduate education at Yale, and Hal Rosvold, with my help, did studies on patients. We would go to state hospitals weekly and study patients who were potential candidates for prefrontal lobotomy, and the notion was that we would do evaluations pre- and post to see what effects the operation had not only on their symptoms, obviously, but also on cognitive processes.

Farreras: Was he the only person you worked with at Yale? Was he your main mentor? Did you have to work on any other research projects?

Mirsky: He was my main mentor, but I did interact with others at Yale. Karl Pribram was at Yale at the time. He was trained as a neurosurgeon and did most, but not all, of the surgery. Operating with Karl was quite an experience. He was kind of a classical, impatient you-fool neurosurgeon who handed me a dull instrument. I remember once he threw an instrument at me because I handed him a forceps that was – the two tips did not quite meet, and he threw it at me and I walked out of the operating room. And Hal Rosvold ran after me. "Come back, come back. That's Karl. He gets very excited. Come back." So I came back.

Anyway, aside from his occasional fits of temper, Karl was a very exciting, stimulating guy to be around. He's still around, teaching at Radford University.

Farreras: In Virginia, yes.

Mirsky: Yes. He was at Yale for a while and then went off to establish a laboratory at a Hartford retreat _____ and then was at Stanford for many years.

So, here's another one, another person, who was Paul McLean [sp.], who was in psychiatry and was a real philosopher of the nervous system, a wonderful guy. Still around, living in the area.

Others included Jose Delgado [sp.], a Spanish surgeon and physiological researcher. These were people who had been, I think, brought by John Fulton to Yale originally to work on this lobotomy project, and Hal Rosvold was one of them. He was the psychologist.

Delgado was a very interesting and flamboyant character. He'd done this famous experiment in which he implanted electrodes into the brain of a bull and went into the bullring with his sword and cape, and hidden was his remote stimulator, and the bull charged at him and he pressed the stimulator and the bull stopped in his tracks. I think there might have been several people waiting with rifles to shoot the animal in case the experiment didn't work. But Delgado was trying to demonstrate how powerful brain stimulation was. _____ that image of him standing there with this stimulator, and the bull... Anyway, he was certainly one of the people at Yale.

There was a tremendous concentration of neuroscience talent: Hal Rosvold, Pribram, McLean [sp.], Delgado [sp.]. In the Psychology Department, Frank Beach [sp.], who _____ research on sexual function, was very famous, was actually also one of my mentors in my dissertations. So I had an interesting graduate experience. It was incredible. That's the only way I could describe it, incredibly wonderful. I had the opportunity to work with patients who were candidates for neurosurgery. I had the occasion to do monkey research involving brain lesions, studying the effects of brain lesions on social behavior and on cognitive behavior. Before we left Yale, we had also started a project involving chimpanzees. I worked some with chimpanzees and was involved with some of that.

I think my fellow students at Yale couldn't quite figure me out, because if I were a clinical student, what was I doing testing monkeys. They didn't quite get it. But in those days... Well, Rosvold was a wonderful, loving, and stimulating mentor, and one of the things that he insisted on was that if you were going to do a Ph.D., you would finish in four years and no question, so I did.

Farreras: Because you went in '50 and finished in '54.

Mirsky: Right. And he set a wonderful example, because during the war, he was in the Canadian Army for five years, and when he came out of the Army, he went to Stanford to get his Ph.D., and he did it in a year and a half. He set some kind of record. So you couldn't argue with him about how long it was going to take to do your dissertation or whatever.

Farreras: Was there an internship year involved at the time that you had to do?

Mirsky: At Yale, it was a so-called externship, which meant that you did it on-site, so I spent six months in the Charles Study Center, six months in the inpatient clinic, the Yale inpatient clinic. I think I must have spent six months in the outpatient clinic as well. Yes. And then six months was sort of my choice, and I chose to work with Janice Stevens [sp.] in the seizure clinic, and she was also a wonderful mentor and part of that stimulating neuropsychological/neuropsychiatric atmosphere at Yale at the time.

Farreras: Yeah. Somebody _____ later publications used patients with epileptic seizures.

Mirsky: Well, that's what really got me into working with patients with seizures.

When I approached Jan to ask her whether I could spend six months working with her in the clinic, she was very receptive to the idea and proposed that we work on a project involving temporal-lobe epilepsy. She pointed out that, at the time, there were many people who believed that there was a kind of relationship between temporal-lobe epilepsy and schizophrenia and that possibly patients with schizophrenia had pathology of the temporal lobes, or, to put it more bluntly, that the temporal-lobe cases were crazier than other cases. There's still a controversy about this issue. But what we did was to compare two groups of patients with seizures, one with temporal-lobe seizures, and a control group who had idiopathic generalized seizures, essentially petit mal epilepsy, and we gave them a battery of tests, including a whole bunch of personality tests – well, some personality tests anyway, THE, Rorschach, and _____ – to see whether or not the temporal-lobe group were matched in terms of all kinds of variables to the generalized group, would look more pathological. That wasn't the case. We found no differences between the two.

The N's were pretty small and we enlarged the N in that particular study when I left Yale with Hal Rosvold and went to NIMH in 1954.

Farreras: So, did you follow Rosvold here? How did you end up coming to NIMH in '54?

Mirsky: Yes. Well...

Farreras: Or how did he...

Mirsky: The 1950s was the era of the Korean War, and Hal was offered a position of chief of the section on animal behavior at NIMH. I'm not sure exactly when the offer came, either 1953 or 1954.

Farreras: I have it he arrived August 16th of '54, and you and Maria Szwarcbart arrived October 1st of '54.

Mirsky: That's about right, that's about right, yeah.

Farreras: Do you know who offered him a position or why he was offered...

Mirsky: I think the position was offered to him by Shakow.

Farreras: Shakow. Okay.

Mirsky: Hal was an up-and-coming neuropsychologist, and his interests in frontal lobe were certainly au courant. People were fascinated, and still are, by what the functions of the frontal lobe are, but _____ in relation to therapy and treatment of disorder.

Farreras: Who came up with the title of animal behavior? Why wasn't it neuropsychology? Or was there any...

Mirsky: I think that it might have been politics.

Farreras: Politics?

Mirsky: Because that's a nutty name, the section, animal behavior. It sounds like a zoo.

Farreras: Was Shakow the one who assigned that name, or was it someone above Shakow?

Mirsky: I'm not sure. But it took years for... Well, when the section flew up and became a laboratory, then it became the Laboratory of Neuropsychology, so whatever political issue had been involved earlier had been resolved or forgotten. I'm not sure.

Farreras: Because it changed to the Section of Neuropsychology in '63, before it even became a lab, so that's nine years of animal behavior.

Mirsky: Oh, good, good, '63. See, I was gone by that time.

Farreras: Then it was neuropsych.

Mirsky: But that was obviously a good change because it wasn't – neuropsychology wasn't pure animal behavior but brain and behavior from the beginning.

Farreras: So it was Shakow who wanted Rosvold here. That was his decision, to bring him in?

Mirsky: I think so. He might... I think with major section chiefs, he probably consulted with Seymour Kety and maybe with Robert Cohen as well.

Farreras: And so, was Rosvold allowed to bring anyone he wanted to bring into the section with him, or...

Mirsky: Yes.

Farreras: Okay.

Mirsky: Now, in '54, when I finished, well, I say with the time of the Korean War, and I had been given this dispensation by my draft board to finish my graduate training, and I think in June of '54, when I got my Ph.D. – the timing may be a little bit off – I think probably that same week, I got the 1A notice from the Draft Board saying, "Please report for your pre-induction physical," which I did in New Haven. And I was classified 1A.

Now, at the time I had been looking at a number of jobs, one actually at Worcester State Hospital, where Shakow was for a number of years, and I interviewed there. And there was also an interview with one of the faculty at Dartmouth, and then there was an alcohol study center at Yale that, there was a position there. And I had warned everybody that I couldn't make any commitment because I thought I was going to be drafted. And people said, "Well, after your tour of duty, get in touch." So Hal, hearing about all of this, said, "Why don't you come and join me at NIMH. We're setting up a new laboratory and it'll be exciting," and I said, "I have this draft commitment." So he looked into it and discovered that there was the Public Health Service, which was one of the two personnel establishments, that you could be either in the civil service or in the Public Health Service, and service in the USPHS, the Public Health Service, qualified as military service since I guess we weren't officially at war. Korea was a police action. The Public Health Service was mobilized, and service there counted as military service. My draft board didn't believe it. I remember Dick Bell, who you will see in Virginia, was the one who sent me the telegram offering me the position.

Farreras: Not Shakow, but Dick Bell.

Mirsky: Dick Bell was, I think, acting for Shakow. He saw to it that my draft board received a letter citing the United States code indicating that service in the Public Health Service counted as military service, because they were prepared to draft me after I got out of the Public Health Service.

So, anyway, so I joined Hal. This happened rather quickly. And so Hal left _____ August.

Farreras: Well, you're right, mid-August.

Mirsky: And so I had the job of packing up the laboratory and sending stuff, sending equipment and books and manuscripts and papers. It seemed to me that I did it all. Probably I didn't, but that's my recollection, boxes and boxes and boxes.

Farreras: Did anybody else join you and him from the Yale program, or were you the only one?

Mirsky: Well, Mort eventually, but that was two years later, and Mort was then with Karl Pribram at Hartford.

I think I might have told you the most interesting part of my shipment of materials from New Haven to Bethesda was the six chimpanzees.

Farreras: I read that in the chapter. But why don't you tell us the story.

Mirsky: We had to figure out the best way to get these six chimpanzees to Bethesda, and we couldn't figure out any other way to do it than to rent a truck, a Chevy Carry-All, which I rented, and we put the chimps into two cages, and the Carry-All was quite a big vehicle and there were two large cages with three chimpanzees each, and we drove at night, we being my brother and I. He accompanied me because we had decided that we would not leave the truck unattended for even a minute. This way we could take turns going to the bathroom and getting something to eat. So we drove down the Connecticut Thruway and the New Jersey Turnpike in the middle of the night, and that was pretty exciting. The chimpanzees were relatively quiet during this time. Occasionally they would make noise. I remember, stopping for gasoline was quite an experience when people, when the gasoline station attendant would look into the back kind of and see these eyes looking back at him. But it really was uneventful, an uneventful trip, smelly but uneventful. So that was probably the most important cargo that I delivered from New Haven to Bethesda.

Farreras: Now, when you arrived... Let me back up. This follows 1954. Who – was the lab already existing at the time? Who had decided to create this lab at NIMH?

Mirsky: I think that was probably Seymour Kety, who was given carte blanche.

Farreras: But that was his idea. And what was the rationale for creating the lab in the first place?

Mirsky: Again, see, I was one of the Indians, not one of the chiefs at this time, and so what information I have about how it came into being is probably what information filtered down from above, and rumors, some rumors, innuendo. But I think I said in the chapter that Shakow was the second choice. The first choice was Robert Claytor Sperry [sp.].

Farreras: Where was Sperry at the time?

Mirsky: He eventually ended up at Cal Tech. Whether he was at Cal Tech at the time, I don't know. Whether he actually came and looked the place over or he refused from afar, I'm not sure. Robert Cohen might know the answer to that.

Farreras: Okay. I'll ask him.

Mirsky: Seymour Kety I'm sure would have told you, although he's very discreet. _____ would want to do anything to diminish Shakow's reputation. But I think that there was this grand plan that the intramural research program would reflect the best of behavioral science, and there was this clinical basic split.

Farreras: Was that there from the beginning?

Mirsky: Yeah, from the very beginning, and it still exists. There are clinical laboratories and basic laboratories, and there was _____. It never seemed to make a hell of a lot of sense to me, but I guess there was the emphasis from the beginning on the fact that it should be kind of an eclectic approach. And, in addition to the _____ basic patient studies, that there should be some basic research on mechanisms that would involve animal subjects. And some labs, it's really hard to tell whether they belong more in basic or in clinical.

Farreras: And for a while there were a few years in the '60s when you weren't here when they were joint, both of them. They weren't separate basic and clinical, and then they separated again.

Mirsky: Well, I think that's probably still true in the intramurals program because Desimone appointed Dennis Charney [sp.] to a major position, which amounts to almost a deputy scientific director, who has many, many positions with a major emphasis on affective disorders. And a big part of the clinical resources are now in Dennis Charney's [sp.] hands, so to speak. And I guess even Bob himself, Bob Desimone, whose reputation is in basic research, deals with that, so there's kind of a de facto separation between the basic and the clinical.

Now, early on each basic research, each scientific director, if he or she was not a clinical scientist, would have a deputy who was more of a clinical scientist to help administer that aspect of the intramural program, so Seymour Kety had Bob Cohen, and there were other examples as well. Seymour Kety had Bob Cohen, and so did John Everhart [sp.]. Bob Cohen contributed with John Everhart [sp.].

Farrera: And he succeeded Robert Livingston after Kety left – right? – as intramural research director.

Mirsky: Yes. Now, Bob Livingston I also knew at Yale. He was another person. But I think his arrival at NIMH was independent of, had nothing to do with Hal Rosvold, although he may have given an opinion about Bob Livingston. Livingston I remember very well because he was the one who taught me how to scrub for surgery. He must have done a few other things as well, but I recall that specifically.

Farrera: And when the lab was created, were there any coordinating mechanisms between the intramural and the extramural program at the time? This was mid-'50s. Or were they pretty much independent the way they are today?

Mirsky: I had the impression that intramural and extramural were a lot closer then. I have this recollection of all of NIMH meeting in one room, and Bob Felix, who was the first director of NIMH, talking about various issues and problems. And I think _____ I think would be important. This was before the establishment of the massive research, sorry, research review structure that we have now, study sections, because I remember from time to time, we'd be asked to review grant applications. But as more and more money was pumped into NIH and NIMH, _____ became obvious. I guess that _____ the same group of people to do basic research and be involved in the administration of grants and contracts so that the separation grew, and extramural moved off campus. And I think that was... My impression is that there isn't a tremendous amount of communication between extramural and intramural.

Farreras: You mean today...

Mirsky: Then the separation led to relative isolation of the two programs, and you might read in the literature about some of the people in extramural, but if you ask the typical intramural scientist if he could identify, or she, the following names, "Who were these people?" "I don't know, never heard of them." "Well, they're in extramural and they collectively administer a portfolio that's several hundred million dollars." "Oh, well, I don' t know about that." Separation between church and state _____.

When I was at Boston University, I remember, I was at the medical school. That's where my laboratory was. But I also, I taught at the main campus, taught students in the Psychology Department, and it was the same sort of separation there, so that people in the Psychology Department didn't know who was in the Psychiatry Department and vice versa, and to try to bring them together, thinking this is a good idea, we're all interested in basic science in relation to behavior, never worked. So extramural and intramural have their own interests. I think there may be some jealousy of people in extramural. They have all that travel money and we have to beg, borrow, and steal _____ all these memos in advance before we can get to go on a trip.

Farreros: Even then, the funding was that disparate?

Mirsky: They always had big travel budgets. I can remember that was an issue. They could go anywhere they wanted. Our travel money was limited.

There's, at this very moment in time, there is this prohibition about travel.

Farreros: _____ travel advisory.

Mirsky: And I was just told that I can't go to the meeting of the American Epilepsy Society because I got my request in only 33 days in advance rather than 35 days in advance and that my name wasn't on the list. What list? So travel has always been something of a major issue. When new administrations come in, they look at travel askance and they say, "This is some kind of big junket," and they don't realize how important travel is, particularly for younger scientists to go to a meeting and to see people and to hear stuff. Older folks can always figure out other ways to get there, off the record. My trip to the...

Farreras: What about the labs? There were three labs before the Lab of Psychology? Neurophysiology, Neurochemistry, and the Social Environmental Studies Lab?

Mirsky: Mm-hmm.

Farreras: Was there any collaboration or any relationship at all between those labs, any contact?

Mirsky: Well, we certainly knew about them. And, for me and others in the section on animal behavior, it would be very exciting to be making contact with some of the people in the Laboratory of Neurophysiology. The first head was Wade Marshall [sp.], who was kind of a crusty, charismatic character, and I loved to listen to him. And he had done some really fundamental research on the functions of sensory areas of the brain, and he was the one who brought Paul McLean [sp.] to NIH. And I believe in Wade Marshall's [sp.] lab, John Lilly [sp.] headed a section.

Farreras: The same Lilly from the Eli Lilly company? Different?

Mirsky: Different Lilly. John Lilly, who did sensory deprivation experiments. He had this tank in which he would pour warm water and continue moving and flowing, and he would use this sensory deprivation experience to study the effects of this altered sensory environment on consciousness, and he was really kind of a quirky but brilliant guy. Also did sensory self-stimulation experiments with monkeys, and he was one of the first to explore the _____ of dolphins, and at one point disappeared from the NIH campus and no one knew where he had gone, but he'd apparently gone to do some research with the Navy on using dolphins as sort of tools to plant bombs or to, on submarines or to do various other kinds of things.

Farreras: Sounds like Skinner's engineering pigeon research.

Mirsky: Yeah. Very charismatic guy. Died about two weeks ago. I saw his obit in the *Washington Post*. But he was _____ Marshall's [sp.] lab.

So there was a fair amount of contact between our section and Wade Marshall's [sp.].

Farreras: And the Neurophysiology.

Mirsky: Yeah, because there was some overlap of interests.

Farreras: Was it collaborative? Was there any tension?

Mirsky: I don't remember there being tension. I think there was _____ enough to go around for all, and the program would grow by leaps and bounds. Every year, there would be more money. I'm not sure exactly how it would be. Not that everybody got 10 percent more, but I guess Shakow would establish other sections or go give resources to other groups. It generally was a period of very rapid growth.

Farreras: Okay. Why don't we jump a little bit to that, then, since you're bringing it up now.

What was the funding like on a broader perspective, between the intramural and the extramural? Was it sort of evenly split, or extramural got more, intramural got less, vice versa?

Mirsky: Well, I think, as of about 10 to 15 years ago – I'm going to extrapolate back – the proportion was that roughly 85 percent of the funding to NIMH was extramural and 15 percent intramural.

Farreras: And you said this was up to 10 to 15 years ago?

Mirsky: Yeah. Now, I'm guessing that as, that initially, it might have been more 50-50, but I don't know. I was sort of out of the loop of that decision-making. But I think that grants and contracts was the part of the program that grew most rapidly, and my guess is that, in the '60s, it was probably the case that that proportion of 80-20 or 85-10 or even 90-10 became established, because the outside world always looked somewhat askance at the intramural program. "Why do we need this for? Why do we have to have an intramural research program? Give us the grants and we'll do the research. We don't need any intramural," so that the leaders and directors of intramural always had to have sort of one eye on the outside world, and they wanted to guarantee that we were not in some kind of ivory tower independent of the pressures of the outside world, but we were doing just as good science as they, and there had been that tension. Tension between extramural and intramural, I don't think so. I think that the scientific director who runs the intramural program probably is always trying to get more money out of the director of NIMH for intramural activities, and I can remember, when I was a lab chief, being party to those discussions just so we could squeeze a few more bucks out, or else we would have to do XYZ.

Farreras: And that was in the mid-'80s when you came back.

Mirsky: Yes. But, see, one of the reasons you came into the intramural program was that you were freed from the necessity of having to write grants and you lived from year to year, that this was going to provide some kind of secure funding for you. Not that you would never have to do any more work, but that you could plan long-term projects that didn't have _____ results every six months that you could report to some granting body. And that changed so that the intramural program was being scrutinized always, and you have the feeling that there are many people looking over your shoulder. "Where do you get off not having to write grants like the rest of us?" "Well, I mean, that's why we came here in the first place." "Well, it doesn't matter. Life is tough out here. Don't you know that?" particularly when money for grants might not be as plentiful as it was the year before. So the role of external review bodies has become more common _____ the last 10 or 15 years, I would say, and it used to be that the intramural director would sort of run things as he or she wished, and outside bodies would be advisory, and that's... Now they have more of a role, I think, and if the intramural, if the Board of Scientific Counselors is not pleased with a particular program, they can terminate it. And the intramural director, I think, probably has to listen. So, the thought that you had sort of a relative sinecure here is long gone, and I think for many people, the strictures of working at NIMH or NIH now are such that, well, stay where you are at the university because you have probably a better shot at getting funding for your research because you have many options. You can apply to various kinds of agencies for money. Right? _____. It's hard to do that here, although there is some of that, as you know. So, the notion of a marvelous, carefree atmosphere in which you can do research without worrying about public or private pressure _____. That may be a little too harsh, but I think certainly the atmosphere has changed. Them's my thoughts.

I think I'm sort of rambling all over the place, but certainly the '50s, it was terribly exciting and interesting. Shakow would urge us from time to time to collaborate more particularly with the Laboratory of Adult Psychiatry. I think you haven't mentioned that.

Farreras: No.

Mirsky: But there was...

Farreras: Who was the chief at that lab?

Mirsky: My recollection is that it was Charles Savage.

Farreras: Okay. I can look that up.

Mirsky: And one thing that was illuminating our work at that time was psychedelic drugs, and this is the '50s and American soldiers were being captured by the North Koreans and Chinese, and there was a question of brainwashing.

Farreras: And _____ CIA-funded research on mind control.

Mirsky: Right, and whether LSD would be a way of getting into somebody's psyche in a way that no other method could provide. There was some work that might have been clandestinely supported by the CIA. I know that for sure. There were some scientist here who was introduced here because the CIA wanted to see his work done, and I think it was almost over Kety's dead body that this guy came here, but that's the way it was.

Farreras: But I think a lot of was also funded through the Josiah Macy [sp.] Foundation, used as a funding front. And there was another one – I can't remember where now.

So, was that Shakow's idea, to collaborate with this lab? Or was there any pressure from above for a psych lab?

Mirsky: Well, I think that the work being done by the Laboratory of Adult Psychiatry was just not very good, and I think Shakow might have wanted to support it, and I figure that some of the behavioral scientists in his laboratory could work with them and support them and see that their work was better, because Shakow is a friend of psychiatry. I think he himself… Well, obviously, _____ did any more psychotherapy but certainly was interested in the psychotherapeutic process, which was in some ways one of the least successful parts of his tenure here. You know about the psychotherapy project.

Farreras: Where he taped an entire psychoanalysis?

Mirsky: Yes.

Farreras: That part, and then they were destroyed for lack of...

Mirsky: Right. Nobody used them. Too bad.

Farreras: What a resource to have nowadays, though.

Mirsky: The _____ somehow, but the technology wasn't available _____ had been converted to some sort of digital format. It wouldn't have been an issue of, what do you do with these 300 tapes that have to be kept in air-conditioned comfort. Oh, well.

Farreras: And the labs. What about the intramural labs? Was the funding... How was that determined or how was that distributed among the actual different labs? Do you know?

Mirsky: Well, in the '50s, I think people submitted budgets, and if they needed... When you came, when Hal came, Hal Rosvold came, he was offered certain space and certain positions, FTEs, and certain amounts of other-objects money. Other-objects money is everything that isn't personnel, so that includes equipment, supplies, travel, consultant fees, whatever. And I think, on a yearly basis, he needed more, he would make a case for it, and if you're lucky, you got it, and if you weren't lucky, you didn't get it.

Farreras: At the lab level, lab chief, at the intramural...

Mirsky: Well, I think there was probably negotiation between the lab level and the director, intramural scientific director level, but I guess technically, everything belongs to the scientific director, and if somebody retires or leaves your laboratory, you don't automatically retain that position. You have to argue for it. And that's the way that your laboratory can be reduced from 20 positions to four, because as people leave, those positions are not restored to you, and there's this general pool if your work is not in favor. So your resources and your success are a function of how well you're regarded by the intramural director, and if you come with the guarantees of one intramural director and he retires, then you are never quite sure what's going to happen.

Farreras: _____ the director?

Mirsky: Yes.

Farreras: Was there any division of funding between basic and clinical? Or were those also sort of similar? Or did clinical get less money than the basic area?

Mirsky: Well, you mean in intramural program generally, or in psychology?

Farreras: Well, back when you had arrived in those late '50s years, when you first arrived.

Mirsky: Well, I think that clinical is, by its nature, more expensive because of the costs associated with beds and patient care. And if you had beds or... Of course, laboratory psychology never had beds, and I think that was something that I grumbled about in the paper that I wrote for Wade Pickrin [sp.]. But I can recall a friend of mine who later became chief of the Laboratory of Neurophysiology, Edward Evarts, telling me that he had been offered a particular section in a laboratory that Kety had established that included – oh, I don't remember exactly – something like 10 beds, and each, to support a bed at that time – and this is in the '70s – to support one bed cost $800,000 a year. So you would have had the responsibility of seeing that $8 million worth of clinical facility was well used, and he said, "I don't want that responsibility." God knows what it costs nowadays to support one bed.

Some institutes, of course, are more expensive than others, if you have super-duper isotopes and very expensive surgeries and so forth, and that's always been an issue whether NIMH is getting screwed by the other institutes because it contributes more than its share in terms of what it uses. But now we use a lot of imaging resources, so maybe we're getting back at them. So I think...

Farreras: It's more costly to do clinical research.

Mirsky: Yes.

Farreras: Right, right. And the sections. Did each get about an equivalent amount of funding?

Mirsky: Well, I think...

Farreras: Because they would also have different types of equipment and instruments.

Mirsky: Yeah. I think probably our section was probably one of the more expensive sections because we had to purchase expensive animals and expensive equipment, and it's different if your work involves paper-and-pencil studies and questionnaires and access to computers. That's relatively cheap compared with buying monkeys and chimpanzees and providing for their care and so forth. So I would guess that our section might have been one of the more expensive ones. I'm talking about the section of animal behavior.

Farreras: Right, animal behavior, the early animal behavior one.

Was there any grumbling from the other sections because one section might get more funding than the other? Or was it sort of rationalized that that's because that's what the equipment...

Mirsky: People generally had what they needed to do, and there could be collaborative work between groups in different sections. But you have to really look at the bibliography emanating from NIH at the time to see how much real collaboration there was, but certainly you were encouraged to work with other groups. And I guess one of my problems was that I worked with people in other institutes. Some people didn't even think that, because of my interest in seizure disorders, that I belonged in NIMH, and, "What are you doing here anyway?" _____.

Farreras: Now, you said that there was enough money for everybody to do the research that they were doing. Was there ever too much money?

Mirsky: Too much money?

Farreras: Too much money. You've mentioned that Felix had been told, "You've got $6 million to work on mental retardation, and you have to come up with something." Was there ever a point...

Mirsky: Well, that was an extramural effort. That wasn't going to be... As I recall, that wasn't going to necessarily filter down to intramural.

I remember. Too much money. That's sort of a dirty phrase. Although certainly in Ed Evarts' case, turning down the position as chief of the unit with the 10 beds, or however many, was more resources than he wanted to deal with.

I remember at one point I wanted to buy a piece of equipment which didn't exist, so it had to be custom-built, and I needed $25,000 for this equipment, and it sounds like such a trivial amount now.

Farreras: That was back then?

Mirsky: Yeah. This was in the '50s.

Farreras: Twenty-five thousand dollars.

Mirsky: Yeah. And so I asked Rosvold for it and he asked Shakow, and so they found that there was some little pot of money that hadn't been used that year and it could be made available. There was this mysterious concept of no-year money, no-year money, which was a fund that I guess intramural directors could access. I don't know whether lab chiefs could get to it or not. Probably. The no-year money meant that this was not dependent on yearly appropriations from Congress, and I don't know whether it still exists, but it was a source of funds that wasn't dependent on an appropriation and that the intramural directors could use when they needed to buy something that they thought was particularly important. But that – you'd have to talk to people who were in the position of being intramural directors to know whether that concept still exists or whether those funds are still available.

Farreras: Was any extra money or any available funding used, was it used with the scientists who were already there to do their research, or was it used to bring in new people into the sections, into the lab?

Mirsky: Good question. I think both. Probably, from my vantage point, which wasn't deeply embedded within the decision-making levels, it seems to me that when more money became available, more people were hired, or people had access to more equipment, more resources. Of course, nowadays the sky's the limit. MRIs and scanners of various kinds cost in the millions of dollars, and if there is a program expansion, you'd buy a piece of heavy equipment and you'd buy the staff that could support it. And it was always the case that it was thought that this would be a general resource for the intramural program rather than for a particular laboratory, though sometimes that was more honored in breach. People would sort of take it over, and it wasn't generally available for anybody who had an idea that would require use of that equipment. But that's my thought.

I think the intramural director and probably the lab chiefs had a fair amount of leeway as to how they would use additional money as long as they were still regarded as fair-haired boys and girls, and their work was considered important and germane and au courant. If you fell out of favor, then you could fall very far indeed. But as long as you were still in favor, in these days it would not be outrageous for somebody to say, "Well, I understand that there's now a five Tesla MRI available, and it's going to cost $10 million, but it will enable us to do studies down to the fraction of a millimeter in terms of our definition." That wouldn't be an outrageous request. Of course, that was the whole budget of NIMH back in the early days.

Farreras: The funding, do you think the research that was done, was that driven by each investigator's particular or personal interest, or do you think that funding might have actually influenced the type of research that was done?

Mirsky: Well, see, the investigators that were invited to come here were chosen on the basis of the overall plan, say, of the lab chief or the intramural director. And if somebody...

Farreras: So in that case, it was Shakow or Kety.

Mirsky: Yeah. And if there were negotiations with, say, a respected lab chief or section chief, you offered this person something, and the person would say, "That's acceptable," or "No, that's not acceptable," and if you needed to make the offer better than was done in some cases.

I can remember this possibly apocryphal story involving Fritz Raydle [sp.], who was the chief of – what laboratory, what branch. It had to do with child development or a type of pathology. He had established a reputation for working with very disturbed, aggressive children and was invited to come here, and he did come here. And I remember, this was the story, that he was shown the fourth floor of the Clinical Center. They said, "This is your space." "Not enough. It's not enough." So Fritz Raydle's [sp.] kids _____ a lot of space, and he felt that _____ wasn't the whole fourth floor, maybe only half the fourth floor. That was the story about Fritz Raydle [sp.]. They really wanted him and they did whatever they could to make it attractive for him, including building various outdoor playgrounds for these kids. Now, there's no space around here anymore for playgrounds, but that's what they were supposed to have, playgrounds. You can go out there and _____ walking through the halls _____ Fritz Raydle's [sp.] kids. Those are the ones who'd spit in the water fountain as they walked by.

Anyway, they could make offers to scientists that were hard to turn down because they were so attractive.

Farreras: Now, it seems you're talking about trying to hire sort of the cream of the crop, people who were doing particular research, versus trying to hire people who would work on... Or maybe it's a combination of the two, whether it was socially relevant, whether you're really looking for scientific excellence versus social relevance as the hiring criterion.

Mirsky: I think it was only scientific excellence. Social relevance... Well, you wouldn't, at NIMH, the National Institute of Mental Health, invite somebody to join who was an expert in entomology. It would be something relevant to mental health. And some people have complained that, in fact, current research programs in many cases are so far removed from basic behavioral and mental health concerns that it's hard to know how relevant they are. But I think that's not fair; that's not a fair criticism. We don't know how basic research on cognitive functions will turn out to be useful in terms of understanding schizophrenia or manic-depressive illness. So you get people who others or you consider to be bright and gifted, and you give them what they think they need and go for it. But I don't think social relevance per se, outside of that context, really was an issue.

I never felt pressured to work on a particular problem because it was socially relevant, but there would be inquiries from time to time. For example, I remember getting a call once that was referred to me. It was from the White House, or, I guess, through the White House, from Tipper Gore: "What studies are you doing on the effects of lead? Are there any studies in the intramural program on the effects?" Yeah, well, in fact, we were doing something with our colleagues from Johns Hopkins. So we could answer, yeah, we were doing something, and this is what we were doing, and if some congressman or somebody in the Executive Branch wanted to know about a certain program, you would answer them. Sometimes there would be a bit of hyperbole in how you would dress up the project, but at our level, it was never, "Well, you should now work on the following project. You now have to drop everything and start to work on terrorism. That's it, no more basic brain research. You have to _____." No one has said that. It may be that some congressional group will set up legislation establishing the National Institute of Terrorism or something, and a group of people will be brought in to study terrorism, and if there are enough in the way of lobbying clout, well, that's how our the Eye Institute was established, the Aging Institute, you name it, how the institutes got their names. But specific... It's always been, "Well, we'll set up another program if that's what we're interested in doing, and we'll fund it," because if you take a given institute and you say, "Well, _____ General Medical Sciences. We think you should be doing more behavioral research, and we'll give you $25 million extra in your budget to do behavioral research," and then two years later, they've done nothing, which I think something like that has actually taken place. You can't push them. But if you establish a new institute with that as its target, then it'll happen. That's what I think.

Now, I think I'm going to have to say hasta la vista ____, senora.

Farreras: Si.